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XXV. *Some Remarks in reply to Dr. DAUBENY's Note on the Air disengaged from the Sea over the Site of the recent Volcano in the Mediterranean.* By JOHN DAVY, M.D. F.R.S. Assistant Inspector of Army Hospitals.

Received May 19,—Read May 29, 1834.

IN the second part of the last volume of the Philosophical Transactions, at the request of the Council of the Royal Society, Dr. DAUBENY has stated his objections to the explanation which I have proposed of the origin of the air disengaged over the sunken remains of the volcano of 1831, as described in my last paper on the subject\*.

I am induced to reply to these objections on the ground that they do not appear to me well founded, nor compatible with the facts which I have brought forward.

In the paper alluded to, after having noticed the composition of the air, which I had found to consist of about 80 per cent. azote and 10 oxygen, I remarked that two views might be taken of its origin; one, that it was of volcanic source; the other, that it was derived from the sea water, and merely disengaged by the heat of the volcano.

The first view, that which Dr. DAUBENY advocates, I could not adopt, as it appeared to me least probable. The minuteness of the quantity of air observed in its ascent by Captain SWINBURNE, rising “in small silver threads of bubbles,” (this is his expression,) seemed very unfavourable to the idea of a deep volcanic source; and the admixture of oxygen with the azote seemed to me to demonstrate that its source could not be deep. For the sake of argument, let us suppose, with Dr. DAUBENY, that the volcano had a submarine communication either with Malta, more than 100 miles distant, or with the nearest parts of Sicily, at least 20 miles distant. Supposing it possible that air could penetrate so far, taking the shortest distance of 20 miles, it appears incredible that it should not be deprived of its oxygen in its passage, especially considering the nature of the matter thrown up by the volcano, containing elements possessing an attraction for oxygen. Whether the whole intermediate tract is imagined to be similarly composed, or only the volcanic region, seems immaterial, as a few feet thickness of such material may be supposed sufficient to deprive of its oxygen a very large quantity of atmospheric air. Had the volcano been supplied with atmospheric air through channels of communication with the land, some indications of such a supply, it might be expected, would have been witnessed during the period of its active eruption. But, as I have stated in my former paper, no indications of the kind occurred: the iron contained in the ejected ashes was generally in the state of protoxide, and the cinders generally contained traces of sulphur, and the gaseous products appeared to be very inconsiderable, and such as might be fairly attributed

\* Philosophical Transactions, 1833, Part I.

to the effect of heat in expelling carbonic acid from carbonate of lime and magnesia, and the burning of the sulphur thrown out in coming into the atmosphere.

The second view, that which Dr. DAUBENY opposes, appears to me to be free from all serious difficulties, and not liable to the objections which Dr. DAUBENY has brought against it. Very minute streams of air, differing only from common air, or the air contained in water, in having less oxygen, are observed rising from a bed of volcanic ashes and scorix a few fathoms below the surface of the sea; and where they rise, the cinders are not black, as they are elsewhere, but of an ochry hue. These were the circumstances of the occurrence. The inference I drew was, that the air was expelled from the water by the heat of the bottom, and that it was deprived of part of its oxygen by the attraction of the black oxide of iron, and its conversion into peroxide. If we suppose that the spots from whence the air rose were the mouths of fissures through which steam ascended, the results, it appears to me, were precisely those which might be expected. What the exact state of the bottom was in regard to temperature, it is impossible to decide from that of the surface; but that it might have been what I have supposed, is most easy of belief. When the volcanic island was last visited, just before its disappearance, and its crumbling masses falling to pieces, from the pressure of the hand or foot, eluding the grasp, and suggesting to the illustrious individual who last landed on it the notion of a magician of the old romance, as I heard Sir WALTER SCOTT relate on his arrival in Malta, even then its sides were still warm, and in some places so hot that Miss SCOTT's shoes were burnt. If so, four months after its eruption had ceased, there is no difficulty in the idea that the shoal it formed seven months after submersion might, in relation to temperature, be what I have imagined.

Dr. DAUBENY in his remarks omits to notice the cause I have considered in special operation for the removal of a portion of the oxygen of the air, namely, the peroxidation of the iron. He combats chiefly the opinion I have expressed, that, generally, air in descending from the surface to the depths of the ocean, will be deprived of oxygen by the action of living and dead matters swimming or suspended in the water. He is of opinion that this is not the case; that it is contrary to the analogy of nature; that it is disproved by the existence at great depths of algæ of an intensely green colour. Were this a well authenticated fact, I should consider it a decisive proof; but I am doubtful of the fact. I have never heard of sea weed having been brought up by the lead from great depths in sounding; in no charts which I have ever consulted is such bottom noticed. And there are certain facts which are hardly in accordance with it; such as the state of iron cannon which have been sunk in deep water during a long period, and have been converted, as it were, into plumbago; such as the preservation of wood under sea water for many years, and indeed for many centuries, in the bed of the sea, but occasionally brought to light and thrown up by storms, sweeping away the incumbent layer of sand. The depths of the ocean, as well as its breakers, may be intended in the economy of nature for other purposes than those of animal or vegetable life. As on those shores on which the waves dash

with greatest violence, sandstone barriers are forming from the deposition, amongst the sand thrown up, of cementing carbonate of lime, set free from its solution by the disengagement of the carbonic acid gas, owing to the agitation ; so, in the greatest depths, deprived of oxygen, vegetable matter may be tranquilly subsiding, and in process of conversion into beds of coal, for which the temperature of the water at its maximum density, judging from the formation of peat, appears to be most favourable.

Dr. DAUBENY objects to the disengagement of air from water under the circumstances supposed, subjected to a high temperature under pressure. He says : “ Either in this situation the pressure of the superincumbent mass of fluid is sufficient to prevent the conversion of the lower strata of water into steam, or it is not. If it be, this same pressure will enable the water to retain in solution its original quantity of air, or at least the greater proportion of it. If, on the contrary, there be not sufficient pressure for the purpose, then no doubt the water will rise up in the form of steam, through the superincumbent mass, along with the air which it had contained ; but, as the temperature of the sea round about the volcano, at least near its surface, is stated by Dr. DAVY not to be higher than that of the atmosphere, it is plain that all the steam must become rapidly condensed, and when it returns to its liquid state, there seems no reason why it should not exert its affinity for the air intermixed, and combine with it as before.” The greater part of this reasoning relative to the effect of pressure does not appear to me applicable to the phenomenon in question. The volcanic shoal over which the air rose, as already mentioned, was only a few fathoms deep : the circumstances may be considered somewhat analogous to those of an experiment in the laboratory for the expulsion of air by boiling water in a retort connected with a pneumatic apparatus. In this latter instance, the air is disengaged and the steam is condensed, and yet the air is not reabsorbed ; nor does there appear to me more reason why it should be reabsorbed in the instance under consideration. Supposing a portion of sea water to be converted into steam in the fissures of the shoal, the instant it comes in contact with the cold water it will be condensed, like the steam from the retort, and will be diffused by mingling with adjoining water : but the air disengaged at the same time will not be reabsorbed ; owing to its comparative lightness, it will immediately ascend, and passing through water already saturated with air, reach the surface.

Dr. DAUBENY supposes that the quantity of air emitted, as observed by Captain SWINBURNE, bore some relation to that evolved from chalybeate springs ; from whence he infers that “ so constant a supply could hardly be derived from such a source as sea water.” Were not the quantity very minute, “ in small silver threads of bubbles,” and varying from time to time in the same place, as Captain SWINBURNE relates, the objection would have weight. But Dr. DAUBENY admits that this minute quantity is very much less than occurs in most thermal springs ; consequently the argument loses its force. Relative to the analogy alluded to by Captain SWINBURNE between the bottom from whence the air rose and a chalybeate spring, it appears to me that

Dr. DAUBENY has mistaken his meaning, and that the similarity he wished to point out existed not in the bubbling of the gas (that not being peculiar to a chalybeate spring), but in the change of colour of the bottom ; for he says where the air came up most plentifully, there “ the cinders (elsewhere quite black) had a rusty appearance.”

As to the inferior degree of interest which Dr. DAUBENY connects with my manner of explaining the phenomenon, it is of little importance. Truth, of course, or an approximation to it, is the end of inquiry ; and the explanation which I offered had the preference, because it appeared most in accordance with the facts. I would not wish to undervalue Dr. DAUBENY's speculations ; but in attaching so much interest to his opinion, that the atmosphere is the source of the gas disengaged, derived through subterranean channels, I cannot help thinking he has had recourse to a difficult hypothesis of little usefulness ; for what facts are there in favour of the idea that volcanic fires are fed like ordinary fires, or in any way dependent on the atmosphere for their activity ?

That azote is often a product, and an abundant one, of extinct volcanos is certain ; but it does not follow that it is also a product of active volcanos. Probably the ammoniacal salts which form in such abundance in certain solfataras, of which that of the island of Volcano is the most remarkable, is owing to a complicated play of chemical affinities, in which atmospheric air, sulphur, alumine, and steam are the elements chiefly concerned.

Dr. DAUBENY concludes his note by expressing the wish that the quantity of gas evolved by thermal springs should be ascertained. This is a scientific desideratum ; but some caution is required how the knowledge so obtained is made a test of the truth of any theory of the origin of the air in such springs. Atmospheric air may be carried down not only dissolved in water, as in the rains feeding springs, as I have dwelt on in my former paper, but it may be also forced down mechanically in froth by the impetus of a descending stream of water, and, so entangled, may find its way to a great distance ; and according to the nature of the strata and channels through which it passes, it may either lose oxygen by the attraction of metallic bodies, or have its oxygen converted into carbonic acid by the action of carbonaceous matter, or it may ascend unaltered. A remarkable instance of this last-mentioned condition presents itself in the springs of the Sava, about a quarter of a mile above Wurzen. It is recorded in my brother's Journal, in which, on the 27th of August 1828, he has written : “ Admired the sorgente Sava,—a number of deep circular holes, with air bubbling through them, and large jets of water, which is beautifully clear.” And on the 30th he added : “ Examined this evening the air disengaged in such large quantities where the Sava rises. It appeared to me to possess all the characters of *common air*, was not absorbable by water, and supported flame in the same manner as common air.”

*Malta April 20, 1834.*